

ART. XIV. *Researches on the Effects of Blood-letting in some Inflammatory Diseases, and on the influence of tartarized antimony and vesication in pneumonitis.* By P. Ch. A. Louis. Translated by C. G. Putnam, M. D. With preface and appendix, by James Jackson, M. D., Physician of the Massachusetts General Hospital 1 vol. pp. 171. Boston, 1836.

This is the last of Louis's publications which has reached this country. It is the only one, so far as we know, which has been honoured with an American translation. We do not hesitate in pronouncing it one of the most important medical works of the present century; and it is so on two accounts. In the first place, it is important for the new and positive knowledge which it gives us in relation to the treatment of some diseases. In the second place, and more especially, it is important as the first formal exposition of the results of the only true method of investigation in regard to the therapeutic value of remedial agents. It might have been supposed, that in the course of some thousands of years, during which blood-letting has been almost universally resorted to in the treatment of certain acute inflammatory diseases, the actual efficacy of this operation would have been clearly and precisely determined. Amid the chaos of jarring and ever changing opinions concerning the properties and application of *all* the articles of the materia medica, physicians had laid the flattering unction to their souls, that the effects of blood-letting in the cure of simple acute inflammation of the lungs, occurring in adult subjects, free from any other disease, were absolutely and accurately known. Here, at least, they had supposed themselves standing on solid ground. Now and then, to be sure, in the lapse of time, and here and there, over the wide and cultivated realms of medical science, might be heard a solitary voice, uttering its anathemas against the murderous practice, and proclaiming that the day of sthenic diseases had gone by. But, with a few exceptions, and with some slight differences of opinion, the real value of this ancient therapeutic measure in the treatment of the disease spoken of above was considered as definitively settled. How far, and precisely how far, this opinion is correct, remains yet to be ascertained. Like most other subjects of prominent and practical importance in our art, it is undergoing, and is destined to undergo, a more severe and rigorous scrutiny than any to which it has hitherto been subjected. The first fruits of this investigation are contained in the book before us, and, deferring some further observations on the different topics suggested by the work itself to the close of this notice, we shall present to our readers, in as concise a form as possible, the conclusions to which Louis and Professor Jackson have arrived.

The first series of cases of pneumonitis or of pleuro-pneumonia, analyzed by Louis, was observed at La Charité, between the years 1821 and 1827. The cases were seventy-eight in number, twenty-eight of which were fatal. The subjects of them were all in perfect

health at the time when the first symptoms commenced. The following tabular view exhibits the relation between the length of the disease on the one hand, and the period of the first bleeding, together with the number of its repetitions, on the other.

1	2	3	4	5	6	7	8	9
10 3	7 3	19 3	19 3	28 2	13 1	24 2	19 2	35 1
12 2	10 2	29 3	12 2	17 3	16 2	12 4	12 1	11 2
14 2	12 2	20 2	15 2	40 2	23 3	19 2	18 1	17 2
		20	22 4	13 2	35 5	18 2	20 3	30 3
		16 3	12 4	21 2	17 2	15 2	13 2	
		17 4	21 2	13 2		27 2	21 2	
			25 3					
			28 4					
			40 2					
			16 2					
			12 4					
12 2½	10 2½	20 3	20 8	22 2	21 2⅓	19 2½	17 2	23 2

The figures upon the horizontal line above the columns indicate the day when the first bleeding was performed; the figures on the left in each column mark the duration of the disease; those on the right the number of bleedings; and those on the horizontal line below show the mean duration of the disease, and the average number of bleedings.

An examination of the foregoing table would, at first sight, lead to the conclusion that the abstraction of blood during the first two days of pneumonitis, had very much abridged the duration of the disease, while, after these two days, it would seem to make but little difference whether blood-letting was commenced a little sooner or a little later.

"But the amount of difference which exists between these two results, leads us to suspect their exactness; and a thorough examination does in truth show, that the influence of bleeding, when performed within the two first days of the disease, is less than it seems to be at first sight, and that in general its power is very limited.

"Indeed, among the cases of the same column in which the antiphlogistic treatment was instituted on the same day, (those of the first and second excepted) the duration of the disease exhibits the greatest variety. Thus in the fourth column, some were convalescent on the twelfth day, others (not to take the extreme) the twenty-fifth and twenty-eighth. This we cannot attribute to the violence of the disease, which was the same; nor to the difference of the treatment, which was equally energetic, and directed by the same physician. Whence it seems to result, rigorously, that the utility of bleeding has been very limited in the cases thus far analyzed.

"Differences no less considerable in the length of the disease would unquestionably have existed among the cases bled within the first twenty-four or forty-eight hours, if their number had been greater. And on the same supposition, the difference of the mean duration of pneumonitis, in subjects bled the two first days, and those who were bled at a later period, would have been less considerable. So that we should get nearer the truth, should we estimate the real difference effected in the progress of the disease by the greater or less promptness with which we have had recourse to bleeding, by taking the mean duration of the disease on the one side, in the cases bled during the four first days: and on the other, in those who were not bled until the fifth to the ninth inclusive. And then the

mean duration of pneumonitis would be seventeen days among the first, and twenty among the second.

"But the average given by the table is probably still a little too favourable in respect to the patients bled within the two first days, for another reason; to wit, that, not having committed any error of regimen before the bleeding, these patients were in a condition the most favourable for treatment; this was not the case with those in whom blood-letting was employed at a later period, and among whom many in each group had committed errors in regimen; some had taken strong drink, such as hot sweetened wine, one or many days in succession, in a greater or less quantity; some had even taken brandy. The length of the disease must certainly have been increased by these errors."

The average age of the patients bled before, and of those bled after the fourth day, was nearly the same. It was thirty-three years in the first set, and nearly thirty-six in the other. Neither was there any considerable difference in the severity of the disease in the two groups of cases. In establishing the duration of the disease, the commencement was dated from the period when the patient experienced a more or less violent febrile affection, followed or accompanied by pain on one side of the chest, and by rusty sputa; the time of convalescence was fixed at the period when the patient began to take some light nourishment; three days at least after the febrile action had ceased; although the local symptoms had not disappeared in every case; that is to say, at a period when percussion of the chest did not always elicit a perfectly clear sound at the part affected, and when the respiration was not very pure, the ear still discovering here and there some crackling and traces of crepitation.

"The facts relative to the fatal cases confirm these conclusions, and seem still further to limit the utility of blood-letting. Out of twenty-eight cases in question, eighteen were bled within the four first days of the disease, nine from the fifth to the ninth; and if on the one hand, we take together all the patients who were bled for the first time within the four first days of the pneumonitis, whatever may have been its termination, and on the other hand all those who were bled at a later period, we have, in the order indicated, on one side, forty-one cases, of which eighteen, or about three-sevenths, were fatal; and on the other, thirty-six, of whom nine, or only one-fourth, were fatal: a startling and apparently absurd result; the explanation of which is found, to a certain extent, in the following table. This table, which relates to the fatal cases only, shows in each of the columns from left to right, the duration of the disease, the number of bleedings, and the ages of the patients; whilst the figure above each column indicates the day when the first bleeding was practised.

1	2	3	4	5	6	7	8	9
6 5 18	53 5 65	4 1 57	29 2 19	16 4 58	32 4 20	20 2 68	25 1 40	22 1 50
	12 3 69	16 2 54	29 4 46	8 2 36	10 2 40			
	8 2 65	6 3 30	12 1 85	9 4 24	29 3 24			
	12 1 55	6 4 47	15 3 37					
	17 7 75	47 2 75	17 1 67					
		11 4 45	20 3 22					
6 5 18	20 3 66	15 3 51	20 2 49	11 3 48	33 3 28	20 2 68	25 1 40	22 1 50

"We see, in effect, that the patients who were bled within the four first days of the disease, with the exception of one in the first column, who was eighteen years of age, were older than those who were not actively treated until after this period, in the proportion of fifty-one to forty-three years: this difference may not seem great, but it may have had great influence on the issue of the malady. Indeed the difference in question, that of age, is much less, if, taking the fatal and suc-

cessful cases indiscriminately, we add together on the one hand all the patients bled within the four first days; and on the other those who were not bled until a later period; for we then find that the mean age of the first class is forty-one, and that of the second, thirty-eight. But it is nevertheless true, that the number of patients bled on the first day, who had passed the age of fifty, was nearly twice as great as that of the patients of the same age, who were bled at a later period. This must have had great influence on the mortality.

"But it is not enough to have studied the effects of blood-letting upon the progress and termination of the disease; its influence on each particular symptom must be separately investigated. Let us begin with pain.

"*Pain* was not arrested by blood-letting in any of the cases bled within the four first days of the disease. On the contrary, it generally increased during the succeeding twelve or twenty-four hours: and its mean duration, usually in proportion to that of the disease, was six days among those who were bled during the four first days; eight and a fraction among those bled at a later period. It yielded more readily to local than to general bleeding.

"The *sputa* regarded as characteristic, were *adhesive, rusty, or like apricot jelly, and semi-transparent*: the mean duration of these sputa varied like that of the pain, or nearly so; being five days in patients bled within the three first days, six in those bled within the three following, seven in cases where the bleeding was from the seventh to the ninth day inclusive.

"The morbid character of the sputa became more distinct after bleeding, in the greater part of the cases in which it was employed at the onset of the disease. On the contrary, the sputa were less morbid on the day following the bleeding among the patients who were not bled until a late period.

"It seems to me, this can only be explained by admitting that the disease had approximated its natural termination in this group, and that it was more or less distant from it in the other. An important fact, which explains the difference of the effect of bleeding in circumstances which are similar only in appearance, and which shows, with many others of the same kind, that we probably do not arrest inflammations at once, as is very generally believed.

"As it regards *crepitation, resonance of voice, agophony, and dulness on percussion*, their ordinary length varied like that of the preceding symptoms; that is, in the cases bled at a very early period, they were still more prominent, during one or more days after the first bleeding, than they had previously been; whereas they diminished rapidly after the first bleeding when this was employed at a later period; at least in the majority of cases.

"The *acceleration of the pulse* continued four, five, six, seven days and more after the first bleeding, in the cases bled from the first to the sixth day of the disease. Sometimes it even increased from one day to another, between two bleedings. The effect of bleeding upon the pulse seemed more decided when we practised it later than the period indicated. That is to say, in a considerable number of cases of this kind the pulse became calm, three days after the venesection; much more rarely not until four or five days. This undoubtedly depended, as was before remarked, with regard to the sputa, upon the circumstance that the bleeding was practised near the time when, in the natural course of the disease, the pulse was about to resume its natural state.

"As was the case with the quickness of the pulse, the *heats and sweats* diminished rapidly after the letting of blood, only when it was done at a certain interval after the commencement. The sweats continued longer than the heat, and lasted proportionably longer than the other symptoms in those individuals who were not bled for the first time until six days after the commencement of the disease.

"Thus, the study of the general and local symptoms, the mortality and variations in the mean duration of the pneumonitis, according to the period at which blood-letting was instituted, all establish narrow limits to the utility of this mode of treatment. Should we obtain more important results, if, as is practised in England, the first bleeding were carried to syncope?

"This practice deserves a trial, but great success cannot, I think, be anticipated; since many cases, the history of which I have drawn up, and which were fatal, were bled to a sufficient extent. Among these there was one who was bled on the day of the attack, and who nevertheless died on the sixth; the vein having

been opened five times, and the quantity of blood lost twelve or sixteen ounces each bleeding."

From 1830 to 1833, twenty-nine cases, suitable for analysis, were observed by Louis at La Pitié. Four of these were fatal. The bleedings in these cases were somewhat more copious than in those observed at La Charité, and the remaining treatment was different in some respects. In the group of patients who were bled for the first time, from the second to the fourth day inclusive; the mean duration of the disease was fifteen days and a half; in those who were not bled till after the fourth day, the mean duration was eighteen days and a quarter, leaving a difference between the cases treated at La Charité and those treated at La Pitié, in favour of the latter. Antimony, in large doses, was taken by many of the patients at La Pitié, while blistering was resorted to at La Charité, but not at La Pitié. As it is our principal purpose in this review to exhibit the results of Louis's and Jackson's investigations in regard to the effects of blood-letting, we must refer our readers to the book itself for their very interesting and important conclusions in regard to the value of other remedial means in the management of pneumonitis. So far as blood-letting is concerned, the following are the results of Louis's researches:

"1st. That blood-letting has a happy effect on the progress of pneumonitis; that it shortens its duration; that this effect, however, is much less than has been commonly believed: but that patients, bled during the four first days, recover, other things being equal, four or five days sooner than those bled at a later period.

"2nd. That pneumonitis is never arrested at once by blood-letting; at least, not on the first days of the disease. If an opposite opinion is maintained, it is because this disease has been confounded with another; or because, in some rare cases, the general symptoms rapidly diminish after the first blood-letting. But then the local symptoms, crepitation, &c., for the most part, continue to be developed not the less for this evacuation."

We shall now give the results of Dr. Jackson's analysis of cases of pneumonitis, treated at the Massachusetts General Hospital, so far as the effect of blood-letting is concerned. The period during which the cases were observed, extends from April, 1825, to May, 1835; and the number amounts to thirty-four. Of these thirty-four cases, three are considered by Dr. Jackson as somewhat exceptionable; we shall, therefore, in the following statements, exclude these cases from our calculations.

In three cases, venesection was performed on the first day of the disease. The average duration of the disease in these cases was $13\frac{1}{2}$ days. The number of bleedings was four times in each case. The average quantity of blood abstracted was $61\frac{1}{2}$ ounces to each case. In fifteen cases, bled for the first time on the first, second, or third day, the average duration was $12\frac{1}{2}$ days. The same thing is true, with a minute fractional difference only, of twenty-one cases in which blood-letting was practiced on or before the fourth day of the disease. In five cases, bled for the first time after the fourth day, the average day of convalescence was the $13\frac{1}{2}$. The average duration of the disease in these twenty-six cases was very nearly $12\frac{1}{2}$ days.

There were five cases in which blood-letting was not employed,

except that in one of them six leeches were applied. The mean duration of the disease in these cases was $14\frac{3}{4}$ days. The results of these accurate data show that by bleeding on or before the fourth day, the disease is abridged in duration nearly one day, when compared with the cases in which this remedy was not resorted to till after that period. Compared with those who were not bled, it was shortened a little more than $2\frac{1}{4}$ days.

"But this," says Dr. Jackson, "would be representing the subject in a light sufficiently favourable to the cause of our remedy; for, in truth, the cases in which blood-letting was not employed were much less severe than the others, taking an average on each side. So that the advantage derived from blood-letting in our practice is greater than that derived from the same treatment in the hands of M. Louis. It may be suspected that this difference is to be attributed to the other treatment employed by us. When all our statements have been made this opinion will not appear very tenable. The average period for all our thirty-four patients taken together was 13.9-34, or 13.26. This is much less than for the cases reported by M. Louis. For this great difference, I think the most probable explanation is that our hospital is much smaller than that of La Pitié; that the comfort of the patients is provided for in every respect better than in the larger European hospitals; and that, especially, there is always preserved in our hospital a higher temperature than in the Paris hospitals. If there be exceptions to this remark among the hospitals in Paris, La Pitié is not one of them, unless I have been misinformed."

It is perfectly obvious, that in order to appreciate accurately the effects of blood-letting in the foregoing cases, all the circumstances which could in any way influence the disease must be taken into consideration, and the real importance of each and every one, as far as possible, determined. This has been done to a considerable extent, both by Louis and Jackson, in the work under review. The operation of blisters, for instance, is investigated by both these gentlemen. Dr. Jackson uses in the treatment of his cases calomel, colchicum, and opium. The circumstances of age, sex, and severity of disease, must also be regarded. The very important subject of temperature will be noticed in the above quotation from Dr. Jackson as one deserving great attention. In connexion with this single point we may state, that Dr. Jackson found his patients to have an early convalescence in proportion as they entered the hospital early after the commencement of their disease. Thus, of nineteen patients who entered the hospital from the first to the fourth day inclusive, the mean period of the disease was a fraction less than twelve days. Of twelve who entered from the fifth to the eighth day inclusive, the average duration was $14\frac{3}{4}$ days. One entered on the fourteenth, and one on the fifteenth day of disease, and the average period of these was twenty-five days. "No other circumstance," says Dr. Jackson, "exercised so great an influence on the period of convalescence as this; so that it would seem to be of less importance, whether our patients were bled or not, than whether they entered the hospital early or late."

It is not our intention to enter upon the other topics discussed in the book. We should not know where to begin; and certainly we should not know where to leave off, short of the last page. The entire work deserves faithful and attentive study. The various subjects of which it treats are so connected with each other, and so

mutually dependent, that the account which it has been our purpose to give of its leading and prominent topic is, necessarily, even so far only as this topic is concerned, partial and imperfect. Let every practitioner who prefers the definite and certain to the indefinite and uncertain,—clear and achromatic vision to the illusions of spectral and shadowy forms,—accurate observation to vague and declamatory conjecture, or fact to fancy, *buy it*.

We cannot let this occasion pass without putting on paper some of the reflections which have been for a long time floating about in our mind, and which have gathered around this book as a kind of nucleus, thus assuming something like form and arrangement. It seems to us a matter of sober and waking certainty, that with Louis's adoption of what is called the numerical system has commenced a new era in our science. The true light has at length shone. The safe and straight path has at last been entered upon. After a vast deal of talking about observation as the only sure guide in medical science, a few men have begun to observe. After ages of preaching the day of practice and example has begun to dawn.

The adherents of the old regime in medicine for a long time looked askance at the pretensions of French pathology. They said that Corvisart, and Laennec and Broussais were engaged in a most frivolous and unprofitable occupation in studying the minute alterations wrought in the organs and tissues by disease. They said, you do not cure your patients any better from this knowledge of morbid anatomy. It is a familiar fact, we presume, to most of our readers, that this has been a standing charge against the anatomico-pathologists, and a standing argument against the value and importance of their labours. There was some truth too in the allegation. It was said, however, in reply, and very justly, that *all* knowledge must be valuable; and none the less so, perhaps, because the precise nature and extent of its usefulness did not become immediately manifest. But we doubt whether even the pathologists themselves, or their advocates, have generally conceived truly the relation of their peculiar researches, and of their results to therapeutics. That their bearing on the treatment of disease is most important, we shall be the last to deny, but we doubt whether its nature is precisely such as it has commonly been supposed to be. In what way does an accurate knowledge of pathology and symptomology of a disease connect itself with the treatment? Does the former, of itself, lead to the latter? We apprehend that it does not. Therapeutical indications do not, in any case, flow from the nature of the disease, as it has been called. A knowledge of the symptoms of any given disease, together with its pathological changes, is necessary solely as a means of certain diagnosis. It leads to this indisputable point, but it leads no further. The treatment of the disease, after it has thus been identified, is to be governed exclusively by the decisions of experience. The diagnosis of phthisis, for instance, is now settled. Its natural history is written. Its specific characters are ascertained. In nearly all cases it is clearly and unequivocally recognisable. But how far is our knowledge of its

symptoms and its morbid anatomy to guide us in our practice? In the absence of all knowledge derived from observation, they might suggest some hints or probabilities derived from uncertain and loose analogies. They could do nothing further. Suppose it should be found by trial that phosphorus, or arsenic, or strychnine, or any other substance whatever, mitigated the severity of the disease, or prolonged the life of the patient, should we be justified in refusing assent to the practice because, according to our *à priori* reasoning, these substances are contra indicated? The symptoms and the pathological alterations of typhus fever,—thanks again to the numerical method of Louis,—are now pretty well made out. Does this knowledge settle the best mode of treatment? By no means. This is yet to be ascertained, not by *à priori* reasoning from the nature of the disease, but by rigorous observation. It may be found that some substance, which, according to our speculative notions, we should expect would aggravate the irritation of the elliptical plates, will shorten the disease, mitigate its severity, and lessen its mortality.

We say, then, that the natural history of a disease is to be studied, in order that we may recognise it, and be able to distinguish it from others, more or less resembling it. The best treatment must be learned from the trial of remedies. It cannot be deduced from the pathology of the disease. And yet practical medicine has suffered as much, perhaps, from wrong notions on this point as it has from the misnamed and misunderstood experience of physicians in regard to the utility and action of remedies. That practice has been esteemed the most philosophical and the most systematic which has based itself on pathological principles. But this procedure differs in no way from that which led some of the old hypothetical systematists to give acids in all cases, on the doctrine that all disease depended on a predominance of alkaline humours. Because our speculative notions in regard to the nature of morbid action may be less absurd than were those of former times, it does not therefore follow that there is less essential error in the system itself. If these views are correct, it is exceedingly important that they should become generally practical and operative. The widest aberrations from reason and common sense of which physicians have ever been guilty, have been made under the influence of this “false doctrine.” The most disastrous consequences recorded in the whole history of our art have arisen from this cause.

We wish to say a few words in relation to the kind of experience just alluded to. And here, in the very book which we have been noticing, there is a beautiful illustration of the necessity and the efficiency of the numerical method. Dr. Jackson himself, one of the most careful, and accurate, and sagacious, and matter of fact observers, did not know the results of his own experience in the treatment of pneumonitis, till he adopted this system, and counted and analyzed his cases. “I had believed,” he says, “that blood-letting after the third or fourth day in pneumonitis was not often useful; and that sometimes it was injurious; but that on the first, second, and third,

and perhaps on the fourth, it both mitigated the disease and shortened it." The latter opinion he found to be true, though it may be doubted whether to so great an extent as he had supposed; the former he had good reason to think erroneous, while his numerical analysis established the important fact, that temperature, together with the longer or shorter residence of the patient in the hospital, seemed to have more influence on the duration and severity of the disease than even the blood-letting itself. If this may be true in such cases as the one adduced, what shall be said of the great mass of experience quoted as authority by those who are our teachers through the press, and in the lecture room? In many cases this experience may approximate, more or less, to the truth; but in some it is altogether fallacious, and in all it lacks precision, positiveness, and accuracy. This remark needs neither proof nor illustration. The whole history of therapeutics is a confirmation of its truth. This false experience has done more than any thing else towards creating distrust and scepticism in regard to the value of our art. How proverbial, even among ourselves, is the deceptive character of medical experience; and how plain is the reason why it should be so.

The study of pathology and symptomatology by the numerical method,—that is, the full study and analysis of all their phenomena, and the relations of these to each other, or, to speak more accurately, the study which has only very recently commenced of the entire natural history of disease—has enabled us to distinguish and identify a considerable number of the more common and important species. We have thus arrived at the first indispensable pre-requisite to treatment,—that of a sure diagnosis. The next thing is to ascertain the value of remedial measures by precisely the same cautious and rigorous observation which has led to accurate and positive diagnosis. This has been begun, and some of its first results are contained in the joint work of Louis and Jackson which we have been reviewing. It is only begun. The questions which these men are investigating are very far from being definitively settled. This can be done only by repeated and continued observation, conducted in the right spirit, and by men qualified for the task.

We may hope that the doubt and uncertainty in which the value of remedies has so long been involved, will thus gradually disappear; and where a new practice is introduced, we shall have a standard by which to measure it—a quick and infallible test whereby it can be tried. The natural history of the disease to be treated being first known, all the circumstances which can effect it being ascertained and appreciated, we have only to demand how far, and exactly how far, the disease has been shortened in duration, diminished in severity, or rendered less dangerous to life by the new method, when compared with other methods of practice previously in use.

Let Louis and all those, both old and young, who have imbibed his spirit, go on in the good work. We have bid them, God speed! After centuries of an existence partaking somewhat of the mixed character of a sickly and rickety infancy, and an ignorant and half crazed

manhood, our science is at last, under their auspices, and by the aid of their great predecessors in pathology, beginning to assume the form and attitude, and steady tread, of intelligent and maturing life. We have full faith that she will ultimately take her true position by the side of her sister sciences, so long and so immeasurably in her advance.

E. B.

ART. XV. *The Philosophy of Living, or the way to enjoy Life and its comforts.* ('A man's own observation, what he finds good of, and what he finds hurt of, is the best physic to preserve health.'—*Bacon*.) By CALEB TICKNOR, A. M., M. D. New York, 1836. 12mo, pp. 334. (Being No. 77 of Harper's 'Family Library'.)

Were we to judge from the titles of various publications which have been ushered into existence within the last few years, we might conclude that this is the very era of 'Philosophy.' We have the 'Philosophy of the Moral Feelings,' the 'Philosophy of Zoology,' the 'Philosophy of Natural History,' the 'Philosophy of Sleep,' the 'Philosophy of Travelling,' the 'Philosophy of Health,' the 'Philosophy of Manufactures,' and, doubtless, others, besides the 'philosophy' before us; nor, if such were our conclusion, should we be altogether in error. A more philosophic spirit, perhaps, pervades the world; but still an attentive examination of these productions would induce us to infer, that there is no marked reason, except fashion, why the caption should be appropriated to many of them; certainly, with at least as much propriety, might the title of Ticknor's work have been prefixed to the 'Lectures on the means of promoting and preserving Health,' by Dr. Hodgkin, or to the 'Principles of Physiology, applied to the Preservation of Health,' of Dr. Andrew Combe; to the latter of which works Dr. Ticknor makes frequent allusions in terms which it richly merits.

If the fact of the appearance of the 'Philosophy of Living,' as one of the constituents of 'Harper's Family Library,' were insufficient to show that the work is wholly popular in its cast and objects, the admission in the advertisement and preface would establish it. Such works are generally excluded from the Review department of this journal, but it may admit of very serious question, whether this ought to be the case, and whether it is not the bounden duty of the *custodes* of science and literature to notice every production from a respectable professional source, in order that the medical portion of the community may be made aware of its character, so that they may influence the *laity* to peruse or reject it, according to its deserts. Yet, more is this necessary, when improper inculcations are to be found, which cannot fail to mislead the reader.

In the case of the volume before us, this may be considered a work